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ABSTRACT

One reason the field of education research is so vague is that there is no specific methodology that belongs to such research. Policy makers and people who appropriate funds would get a clearer idea of what research can contribute to an understanding of schools, colleges, students, and teachers if the research community were clearer about what it does. The volume and diversity of the products of research on education have not helped the effort to improve the quality of that research. A major problem for education policy-makers and practicing educators and for persons who use their skills in other disciplines to seek new truths about education is their inability to communicate with each other. The problems that worry educators and for which they seek definitive answers through research are vastly complex and cannot be compared to technological problems. In education the fundamental units that are dealt with are individual human beings whose behavior is influenced by differing inheritances, by varied experiences in life, and by feelings and attitudes that are unpredictable and changing. Information about human beings cannot be fed into computers with the expectation that the calculations about them will have the predictability that the laws of gravity will produce when fed into the same computers. Social and behavioral sciences have their limitations in the field of education. (SK)

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Education Research--The Promise and the Problem

by Harold Howe II
Vice President for Education and Research
The Ford Foundation
At Meeting of the A.E.R.A. April 21, 1976 in San Francisco

Of all the subjects that I know very little about, the one that has caused me the most trouble has been education research. The reason is that I, like many others who have had various administrative and policy roles in education, have been unable to ignore it. At the same time I have been continually perplexed about what education research was suggesting can be done to improve schools and colleges and to enhance their services to students. In spite of this puzzling aspect of education research, it appears to be a growing enterprise with more participants and more publications every year. And although, as is already evident, I have some reservations about this mushrooming establishment, I have a growing suspicion that its persistant presence is at least important and perhaps even useful.

Having uttered this pronouncement as my basic judgment about an activity that occupies the time of most of you gathered here, I feel like a slightly perfumed skunk at a lawn party. Perhaps the best thing I could do now would be to creep off into the bushes and listen to the uproar about what I have just said. But before I do that, let me take a few moments of your time to comment first of all on the nature, quantity, and quality of education research and

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secondly on the problems of communication among educators, researchers, and others concerned with education. While these remarks may be of little or no use to education researchers, they could help those who are trying to understand them and might even lead to greater acceptance and support of their endeavors.

I am grateful to your organization for giving me the opportunity for these observations and for allowing me the privilege of your platform. Perhaps the way for you to regard my presence here is in the spirit of an expression of Lyndon Johnson who, when pressed by advisors not to appoint a certain controversial person to an important job, observed "it's better to have him inside the tent spitting out than outside the tent spitting in."

The first, and perhaps most basic, question I have is whether there is any such thing as "education research." Certainly there is no special methodology that belongs to such research. I doubt that we here could agree on a set of concepts or theories that would give it a coherant body of knowledge. If these observations are correct, wouldn't it be a good idea to clear up this muddle and to stop acting as if there is a separate discipline with its own mystique called "education research?" My guess is that policy makers and people who appropriate funds to support research on subjects related to education, people ranging from Congressmen to local school board members, would get a clearer idea of what research can contribute to our understanding of schools, colleges,

students, and teachers if the research community were clearer about what it does. And that, it seems to me, is very simple--it uses the tools, concepts and theories of inquiry provided by numerous disciplines to examine various aspects of education.

Research in education gets done no differently than research on any other broad topic by using different ways of knowing to discover new knowledge. Classified in oversimplified fashion, these ways of knowing break down into three types based upon the division of the disciplines into the natural and physical sciences, the social and behavioral sciences, and the arts and humanities. Each of these has different "ways of knowing," although there are important common elements among them and blurred distinctions separating them. So when we talk about education research, we are really talking about what specific disciplines--biology or sociology or history, to use one example from each way of knowing--can discover about education.

Following this line of reasoning, I am not willing to admit that there is such a thing as an "educationist," although I have, upon occasion, been accused of being one. The only definition I can think of for such an animal would be that an educationist is a person interested in education but without training in a discipline that would provide a way of knowing about it.

This argument may seem to you painfully obvious and essentially unnecessary. But I submit that it has important implications both for the training

of those who engage in research about education as well as for the support and nurture of their work after they are trained. It has implications also for the quality of the work done in the name of education research.

In saying these things I am not trying to stir up a controversy with those who are busily advocating a "science of education," meaning thereby an enhanced professionalism among those who examine seriously the broad field of education. I would be the last to want a confrontation with Piaget, whose work I admire and whose institute we at the Ford Foundation have supported for many years. At the same time I must call to the attention of those advocating a science of education an article in the Public Administration Review for April of 1976 entitled "Toward a Science of Baseball." There they will find the following observations reported:

> "Given the nature of our times, it should come as no surprise that the baseball profession, with a grant from NIMH [National Institute of Mental Health], seeks to make the playing of baseball into a pure science. Accordingly, the baseball managers, players, owners and fans have met with a distinguished group of scientists from many disciplines. Included among the scientists were physicists, chemists, physiologists, psychologists, scientific managerists, political scientists, management scientists, welfare economists, public (collective) choice specialists and others.... They [the physicists] were confident that a theory of baseball could be constructed using basic Newtonian laws of mechanics and motion.... There was much enthusiasm expressed for this solution, and a motion to adjourn the meeting was entertained. But a representative of the Sine Nomine School of Scientific Management...made the point that...time and motion studies could scientifically determine the 'one best way' to steal a base--or anything else the game involves... [and] that scientific management was necessary to theory construction....At this point the physicists inexplicably arose and left the conference center and

> never returned....The owners of the baseball teams took exception to the 'winning' objective postulated by the management scientists. They said that the objective of baseball was not to win games but to make money by selling TV advertisting time.... The psychologists, sociologists, social-psychologists, organizational-psychologists, and the organizational-social-psychologists stressed the need that any model of baseball must take into consideration the human heirarchy of need....In the midst of all this criticism the management scientists walked out of the meeting. They later formed their own organization--The National Institute of Baseball Management, Administration, and Policy Sciences, Etc. They also received a grant from NIMH....The collective (public) choice scientists...claimed that by improving on welfare economics they could construct a theory of baseball that would bring optimum distribution of happiness to everyone concerned....Players would be given show biz lessons and assured lucrative TV contracts....The government through NIMH would subsidize any team operating in the red [to please owners] ....In the midst of the turbulence that followed, the famous baseball player, Shirley Terreberry, was heard to ask, 'Who's on first?' "

Nor am I seeking an argument with those many persons who write articles drawing analogies between medicine and education in the hope that some of the prestige and financial rewards of the former will rub off on the latter, although I think that these analogies are frequently pushed too far. I am simply saying that for understanding education and for analyzing its numerous issues, ranging from how to pay for it to how it inculcates values, the strength of research activity lies in the traditional disciplines of the sciences, social sciences, and humanities.

This argument of course should not lead us to believe that what goes on in a classroom or in the places where debates on educational policy are being

conducted consists of phenomena perfectly amenable to what different disciplines have to say. To cite just a few examples, the economist's vocabularly does not include the word perception, the psychologist's does not deal with resource allocation, the biologist's with power and the political scientist's with motivation. Yet all these concepts are important when we deal with education. But the day of rigorous interdisciplinary research has not yet arrived, I am afraid, and limited as the views of specific disciplines may be, they do seem to give us considerable insight. No doubt we shall grow more skillful in the future in the difficult task of integrating the work of several disciplines into results with a meaning that reaches beyond the sum of its parts.

Where does this viewpoint leave all the people who hold degrees as Doctors of Education? I hope that it has them slightly disturbed, but not necessarily considering suicide. They are persons who know a considerable amount about education, having studied it in an organized way over a period of time. They should be useful in training teachers, advising on policy choices, and generally seeing that schools and colleges proceed with an adequate knowledge of their past sins and current problems. Also, they should be useful on the research front, but their usefulness in research will be determined in large part by their mastery of one or more of the scholarly disciplines.

A good historian like Lawrence Cremin or David Tyack can illuminate education through research primarily because he is a competent historian. Clark Kerr is first of all an economist and Scotty Campbell a political scientist.

It is fortunate that they have applied much of their energy to education. Ralph Tyler, who has waded in the waters of education for so long that some people consider him an educationist, is acquainted with many disciplines, but he started as a psychologist. That discipline provides the touchstone for the validity of his research. James Coleman is a sociologist; Kenneth Clark is a social psychologist. Some of these persons have more than a nodding acquaintance with several disciplines, but all of them mastered one first and grew from there.

People who can make a first-rate contribution to research on education, or on anything else for that matter, come to the job with a first-rate set of tools and a disciplined way to think about problems. One reason that there is such a large volume of second- and third-rate research about education is that there are a great many people engaged in that work with dull tools, or no tools at all, or in a state of confusion because they are attempting to use many tools simultaneously and haven't mastered any of them. One of the potential hazards of becoming a Doctor of Education is that the processes by which such doctorates are created lend themselves to the dilution of the disciplines.

Education holds no monopoly in the field of poor research. Fuzzy work goes on in the sciences and other disciplines as well as in other professional fields such as medicine and religion. But I have the distinct impression that most of these other professional fields and practically all disciplines have

managed better than education to recognize what is shoddy and what is not. In recent years, as one of my extracurricular duties for the Ford Foundation, I took an excursion into medicine, when we made a large grant to a hospital. In overseeing this exercise, I discovered the refreshing fact that people engaged in second-rate research in medicine knew they were doing exactly that. I don't find the same open recognition of levels of quality in research on education.

But other activities besides inadequate tools and theories have encouraged poor research. Let me plead guilty to participation in two of these that have made their contribution to enhancing the amount of research on education without doing as much as they should have to develop its quality. The first is the rapid increase in the 1960s of federal support for research on education, and the second is a special byproduct of that support known as the ERIC system (ERIC stands for Education Research Information Center).

The story of the growth of the federal initiative in supporting research on education is yet to be told in any comprehensive way. Bits and pieces of it have appeared in articles and books. Milbrey Wallin McLoughlin's recent volume, Evaluation and Reform, has some interesting sidelights on the uses and abuses of research by the federal bureaucracy as it confronted evaluation problems. The Congressional hearings on the National Institute of Education provide a critique of what had gone before. I can't even pretend to cover all this ground, but I do have some impressions with the advantage of hindsight.

The strongest of these is that the United States Office of Education moved too rapidly to large-scale support of research activity and was not able to contrcl quality in the process. This should not be taken as an argument for restricting the present budget of the National Institute of Education. That agency is working its way toward disciplining its grants in a fashion that was not typical of the research program of the Office of Education when it built rapidly to well over $100 million a year in the late 1960s and early 1970s.

But returning to my generalization that the federal effort started too large and failed to build a base of high quality research, I have to say at the same time that in the political atmosphere of the 1960s it was next to impossible to do anything else. A carefully designed, national program of educational research drawing upon leading scholars in the disciplines who would apply their talents to educational issues and slowly build from around $25 million a year to something over $100 million was just not the way things got done in the Johnson administration. Big ideas and big money and big promises about results were the order of the day. Thinking about the past record comparatively, I can argue that all the money appropriated for education research in the 1960s scarcely equalled the cost overrun on one weapons system in the Defense Department and raise a question about why anyone should worry about small waste when large waste was so evident. But after all these excuses, I still have to say as a somewhat chagrined Monday morning quarterback, that we could have built a stronger foundation in the early years.

My one comment on the ERIC system is that if it had started with a stronger element of quality control and less concern with coverage, it would now be a better show. Not that it isn't useful, and its brief studies on particular issues are excellent. But at the touch of a computer button, one learns more than one cares or needs to about what's in print on most education-related subjects. I realize, of course, that there is a process of selection for what is reported by ERIC. I wonder whether it would be possible to develop a process of grading.

A second reason that education avoids separating the gold from the dross in its research activities is found in its efforts in the 1960s to include everyone in education. This had its impact in the research field and resulted in the false notion that a very large number of persons could do quality work in research on education. This fallacy still persists, although it has been diluted to some extent by the dismal results of promoting wide participation and egalitarianism in an enterprise that depends upon intellectual power and well trained expertise in the major disciplines. Education research has invited the Indians to mingle with the Chiefs and in doing so has lost some of its capacity to distinguish between them.

More importantly the volume and diversity of the products of research on education have not helped the effort to improve the quality of that research. I have the distinct impression that scholarship in physics, in economics, in art criticism, or in medicine, to name only a few fields, is more effectively self-

regulating than in education. The networks of criticism in these arenas operate more effectively; the scholarly journals may be as numerous as they are in education, but I suspect that there is a much clearer understanding about which of them are significant.

This discussion implies that people engaged in research on education have a job to do in putting their house in order. My impression is that some of them know it and that particularly at the National Institute of Education there is an organized effort underway to identify high quality work and to build upon it. That job is not made any easier by the pressure exerted through the political process not to spend federal funds for education research where the greatest talents are but rather to spread them around so that everyone can have a little. An often heard argument that accompanies this process is that education problems in the United States are unique to particular places in our country so that education research services are needed in each region. This argument gets extended to embrace the uniqueness of states and of cities within states. There is some truth in it but not as much as we have made ourselves believe, and the net result is to sprinkle scarce funds over wide areas where too high a proportion of them are used badly rather than concentrating more of them where there is talent of high promise.

A major problem for education policy makers and practicing educators on the one hand and for persons who use their skills in various disciplines to seek new truths about education on the other is their inability to communicate with

each other. Economists, psychologists, and sociologists necessarily have their own special vocabularies to convey concepts accurately and to lend scientific validity to their work. In addition many disciplines employ mathematical analysis with growing sophistication to produce new insights into problems and issues in education. Neither the special vocabularies nor the mathematical analysis are understandable to a high proportion of the persons who conduct or who are responsbile for the day to day business of education.

There is, of course, nothing unique about this situation. Most of the people who work in hospitals and public health agencies or who are responsible for the funds and policies that advance health programs haven't the foggiest notion of what researchers in the bio-medical field are doing. The same is true of the application of science to military affairs. But the relationship between research and educational activities is expected to be different by the practioners and policy makers in that field. They assume that research should be relevant to the education system as they know it, and they assume further that it should be understandable to them. When it doesn't provide clear answers to issues that they confront or when they cannot understand the answers it provides, educators and education policy makers tend to be both more critical and more suspicious of the research enterprise than their counterparts in other fields.

One reason for this state of affairs is that everyone is intimately acquainted with the education system and has his own judgments about it. Most

of the persons working in or trying to decide about education have, after all, spent sixteen or more years in direct and intensive contact with schools and colleges. They think they are authorities! So research studies that don't agree with their views or that are not clear or that can be understood only by specialists are to them particularly annoying. In this sense research about education operates in an especially troublesome environment, while research about oceanography or China or the solar system or the behavior of animals is relatively free to go its own way.

Another reason for suspicion of the education research community is that it has insisted on making trouble for itself by adopting a pseudo-scientific way of talking about its own affairs. Since the economists, sociologists and others seem to gain status by having their private idiom to communicate with each other, educators without a solid abase in a true discipline have developed a language known as pedeguese or educationese. It is filled with unnecessary verbiage; it has none of the rigor of the special communication systems that good social scientists employ in dealing with each other; and it consitutes the highest form of obfuscation practiced in America today, with the possible exception of the annual reports of corporations and the periodic predictions by financial authorities of the future behavior of the stock market.

Educationese employs words like "ongoing" applied indescriminately. It is dull and totally without merit. Moreover, it is singularly unimaginative. Instead of creating its own expressions, it adopts cast-offs from elsewhere.

Educators discovered the word "thrust" after it had been worn-out by everyone else. It now permeates their dialogue. According to a publication I saw recently, "the winner of this year's THRUST award" is the sentence, "The thrust of this paper will be toward two foci." I wonder whether education research writers will ever return to using the word "classroom." It has been replaced in their writing by the pretentious phrase "classroom situation," which adds no meaning but serves to persuade its user that he has said something more "meaningful," to use another favorite. But these are common sins of many writers. It takes education researchers to devise such words as "directionality," "exponentiation," and "allocentric."

While speaking in this vein, I do not want to let the social and behavioral scientists escape a few jabs. I am quite prepared to recognize that their special fields require special vocabularies and that their methods of analysis have implications for both the style and content of their communications, but I firmly believe that they regularly commit many of the sins I have attributed to educators. Prolixity is endemic among sociologists, and much of it is unnecessary. Whenever I see the prose of the great sociologist, Talcott Parsons, I think how much greater he might have been if he had employed an editor.

But simply suggesting that educators and researchers use good English is not enough. There is a genuine communications gap that has serious implications for both the support of research and for its use. This gap is illustrated by that controversial, large-scale research effort known as the

"Coleman Report." That document has been used to oppose the desegregation of the schools, to argue that more funds for education would not improve learning, and to support the view that schools don't have much to do with learning; it has been used also to defend the exact opposite of these three propositions. It is, indeed, an all purpose document.

Because I happened to be the then United States Commissioner of Education, it was my job to hold a press conference on this report at the time of its delivery to the Congress in early July of 1966. Not long ago I encountered a member of the press who said he had never understood why I had seemed nervous at this particular press conference when I never seemed to be at others. The answer was easy; I was conducting a press conference about something I didn't understand.

One solution to this problem is simple. Don't appoint ignorant people as Commissioner of Education. But the matter may not be that simple. The Commissioner of Education is one easily replaceable bureaucrat, but many of the people who work in the schools and who train teachers are more or less in the same boat. Looking at them from the point of view of a serious researcher, F. Raymond McKenna had this to say in an article in the February 1976 Phi Delta Kappan: "The teacher-education establishment does not know what to do with disciplined, scholarly research in education." I think he is probably right, but to unload all of the "teacher-education establishment" along with the ignorant Commissioner is going to make a big problem.

Just one further anecdote on the Coleman study of 1966 to suggest that the problem may be one that the scholarly researchers can't solve either. After delivering the study to the Congress, where no attention was paid to it until Senator Ribicoff discovered it as a potential source for some lively hearings some months later, I started to wonder whether the Commissioner (still me) should be doing anything about it. Since I couldn't figure out what it said, I invited some scholars to advise me. I have now forgotten the names of the half dozen people who spent a day in Washington in the summer of 1966 trying to answer the question, "Should the Commissioner of Education do anything different from what he has been doing because he has the results of Jim Coleman's study?" I do remember that Professor Philip Hauser of the University of Chicago and Professor Daniel Moynihan of Harvard were in the group. But whoever was there, I found no agreement about what to do next, and the only clear advice I received was that "more research was needed." Pat Moynihan, always an entrepreneur, was awarded a Carnegie grant to have that research done at Harvard through a seminar. The result was a book edited by him and Frederick Mosteller, On Equality of Educational Opportunity, 1972. Perhaps there is a lesson in the fact that the clearest statement I have seen about the Coleman Report and the Moynihan, Mosteller book was done by a newspaperman turned researcher named Gerald Grant. My suggestion to anyone who wants to find out what Coleman, Moynihan, and Mosteller have to say is to read Gerry Grant's article in the Harvard Educational Review of February 1972.

In addition, Gerry Grant's piece is a wise overview of the problems of using survey research to examine policy issues in education.

Enough reminiscing! It won't solve the problem of communication between scholars and practitioners. This is a problem that all of us concerned about education must keep in mind and worry about. There are no quick solutions to it. It has its counterparts in other realms of research and action, but as I have said already, I think that the difficulties in education are especially detrimental to the interests of both research and the institutions and students it ultimately serves.

We in the Ford Foundation sought a partial answer to this problem by a grant to an outfit called the National Academy of Education, a self-appointed group of able scholars interested in many aspects of research in education. The grant is intended to help the Academy assist the public better to understand such research. One of the activities the Academy launched with these funds is the publication of special and timely reviews of significant education research documents. So far its efforts have resulted in a series of pamphlets with many of the same problems of communication the original research had. Maybe we should have given these funds to some journalists.

Those of us who engage in research on education and those of us who support it with either public or private funds should be more modest than we have been about the potentials of such research for solving all the problems of

schools, colleges, and students. The "can do" syndrome that characterizes research and development in technological fields has infiltrated research on education. If we can go to the moon through the miracles of R&D, then why on earth can't we figure out what is the most advantageous class size or the results of increasing expenditures on schools or the effects of integration or how to teach children to read?

The answer is, of course, that these problems which worry educators and for which they seek difinitive answers through research are vastly more complex than the relatively simple matter of going to the moon. In education the fundamental units with which we deal are individual human beings whose behavior is influenced by differing inheritances, by varied experiences in life, and by feelings and attitudes that are unpredictable and changing as life experience changes. Information about human beings cannot be put into computers with the expectation that calculations about them will have the predictability that the laws of gravity will produce when fed into the same computers. Even when large samples of them are used along with sophisticated calculations to do what is called "controlling variables," the resulting calculation has no significance in dealing with the individual and limited value in dealing with groups.

This is not an argument that the social and behavioral sciences are either useless or unscientific. It is simply a statement about their limitations

in the field of education. These are too frequently forgotten both by enthusiastic researchers and by naive laymen who are overimpressed with the scientists' mumbo-jumbo. Sometimes this combination of enthusiasm by researchers and simplistic response from laymen holds dangers for education that researchers may not have intended but for which they must be held responsible. One of the best examples of such a situation is found in the way Christopher Jencks' book, Inequality, was put before the public. A front page article in the New York Times emphasizing Sandy Jencks' view that luck and personality factors were more important than education in producing differentials in income was rapidly parlayed in popular articles and editorials across the United States into arguments that schools didn't matter and that money spent on schools was wasted. Coming at a time when the economy was in trouble and when schools were facing new competition for public funds from other domestic social programs, the initial result of Jencks' work was to hurt children. The counter-fire from knowledgeable critics like Henry Levin and Thomas Pettigrew did not appear in time or get the public notice to undo the harm that was done. Eventually Jencks himself got around to writing an Op-Ed piece in the New York Times to say that he did not favor reducing school expenditures.

I might add that the public announcement of Jencks' findings came at a time when the book was not yet available and was probably written by reporters who hadn't read it or even seen it. I can recall being asked by a national television network program to discuss the book the day after the Times' article

appeared. When I declined on grounds of not having seen the book, it was suggested that I simply read the piece in the Times.

I expect that there are some lessons in this experience for all of us--educators, researchers, and the so-called "professionals" of the media. It turned out, of course, that Jencks' main point was that education is not a very efficient way to redistribute income compared to taking money away from the rich and giving it to the poor. There are, however, some other matters to discuss besides education in connection with this more efficient approach to the redistribution of wealth.

Particularly when we are addressing the large policy issues in education, "other ways of knowing," to use a phrase from earlier in these remarks, frequently become more important than the social sciences. Consider, for example, the Supreme Court decision of 1954 that it was unconstitutional for states to separate blacks from whites on the basis of race. While supporting evidence from the social sciences about the effects of school segregation gave credibility to the decision, it was based primarily on legal, moral, and humanistic views with a long history. James Coleman in reflecting on the relationship of social science research to school integration made a comment reported in the August 24, 1975 New York Times Magazine:

> ..."Let's suppose the 1966 research of mine had come out with the opposite conclusion--namely, that black children did worse in the predominantly middle-class schools. Should the courts have used that as an argument? I cannot envision a decision saying that segregation is constitutionally required because black children do better in segregated classrooms."

The major decisions about education are controlled first of all by the values that predominate in a society and only indirectly influenced by research studies. But that indirect influence can still be important and should not be unduly downgraded. Think, for example, of the extensive research that has been done on the mental, physical, and emotional development of children. I don't know whether this work is legitimately called "education research" or not, but it does seem to me that the halting process of changing school practices to take into account what studies of child development reveal is one of the better illustrations of how research can improve education. The time lag between new findings about human development and changes in the institutions that are in the business of developing humans (schools and colleges) is totally different from the time lag between discoveries in the physical sciences and their application to human affairs. The resistance of institutions to change accounts for part of this lag, but much of it comes from the difficulty of translating research findings into forms that the schools can use. I would be willing to bet that if you were to ask the elementary school principals of the United States what Piaget has to say that might be of importance to their schools and what changes they might make if they decided to reflect his views about children in their programs, most of them would have little to say on the subject. If this is true, it is not solely a critique of school principals.

Much of what I have said here assumes an instrumentalist approach to research on education. There is, of course, a powerful argument that scientific research should be primarily for the improvement of science. I do not want to deny the importance of that mission. But I do want to close these remarks with three assertions: 1) There is an important job to do in improving the quality of the activity that goes under the banner of "education research;" 2) If the people engaged in this activity want support for their efforts, they will have to give some attention to making those efforts produce demonstrably useful changes in education; and 3) They won't succeed in the mission of causing useful change unless they learn to communicate with the people who must carry it out.